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SAMPLE SURVEYS FOR THE SOCIAL ACCOUNTS OF THE HOUSEHOLD SECTOR

PART I—GENERAL PROBLEMS

Social Accounts of Cambridgeshire

We shall discuss in this paper certain general problems which arise in connection with the use of sampling methods for the collection of social accounting information from households and individuals, and describe in a certain amount of detail some field work we have already carried out. This work forms part of a wider research project ' The Social Accounts of Cambridgeshire,' the object of which is to test the use of sampling methods for the collection of social accounting data relating to the economy as a whole. Cambridgeshire has been chosen for the area of study largely on grounds of convenience, but we hope to develop methods which will be applicable on either a regional or a national scale. It is clear that a first step, before any sampling methods can be decided upon, is to group together the very large number of economic entities which exist into sectors and sub-sectors of a reasonably homogeneous nature, and that each one of these will present its own particular problems.[1] ' Households and Individuals ' is one of three main sectors with which we are working at the moment, the other two being ' Business Enterprises ' and ' Collective Providers (Public and Private).'

Our object for the ' households and individuals ' sector is to build up estimates of a year's total income and expenditure for all persons normally resident in Cambridgeshire, in such detail that we can then construct an operating, an appropriation and a resting (or capital) account for the sector, which will show the transactions between this and other sectors and sub-sectors. It is clear that we cannot collect the data in the precise accounting form in which it is required. In fact the approach we shall have to use will be very similar to that used in ordinary family budget investigations—total expenditure broken down into certain categories, and divided between inside and outside Cambridgeshire, income from all sources inside and outside Cambridgeshire, and changes in total net indebtedness, all for a period of one year. We shall consider some of the main problems under the following heads—the sampling unit and sampling frame, the form in which the material is to be collected, and the method of collection.

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[1] See ' Uses of Sampling Methods in National Income Statistics and Social Accounting ', Richard Stone, J. E. G. Utting and J. Durbin. *Review of the International Statistical Institute*, 1950.

The Sampling Unit and Sampling Frame

Britain is fairly well equipped with frames for sampling individuals and households. For a full discussion of the relative merits of the electoral register and the rating lists reference should be made to papers by Moser[1] and by Gray and Corlett.[2] Either of these lists can be used to draw a sample of individuals or of dwelling units, although there are strong arguments on grounds of convenience and simplicity for using the electoral register for a sample of individuals and the rating list, where available, for a sample of dwellings.

There are however strong arguments against the use of the individual as the final unit of analysis in work of the kind we are now discussing. Many of the activities of individuals are in fact conducted through the medium of a group. Much consumption is done via the ' household ' or family, saving is often family saving, and income is family income. It would seem that to use these ' groups ', suitably defined, as our unit of collection and analysis, would tend to yield rather more homogeneous units. It is quite possible that one would need to use different groups for the different functions of expenditure, income and saving. For instance for expenditure one might use the ' household ' defined to include all those people who share common housekeeping expenses, for savings and income, a ' savings unit ' such as is used in the Surveys of Consumer Finances[3] or by the Oxford Institute of Statistics.[4] Provided that certain basic information for classification was obtained these different groups could then all be combined for the final estimation.

In effect this is trying to eliminate the variances which arise because of the different functions of individuals in the group—chief spenders, secondary spenders, chief earners, non-earners, etc. It does not of course reduce the variances which arise because of variations in the size of the group, which are present whether one samples 'groups' or individuals. The possibility of using some method of estimation which allows for this variation is discussed in more detail in Part IV. Apart from the homogeneity argument, there is yet another reason for making the unit of collection the natural group. This is largely precautionary. Where a group of people normally incur some common expenditure the answer to the question ' who buys what ? ' may be rather indeterminate. If only one member of the group is interviewed there is the possibility of deliberate transference of expenditure for purposes of concealment, and, even more important, dangers of double counting and of unintentional omission. There is a clear problem of this kind where certain of the household expenses are paid by cheque drawn on a joint banking account of husband and wife, and somewhat more obscure difficulties where husband and wife make a joint shopping expedition to buy household equipment or the man's clothes. While it is possible to make rules which will cover such cases in principle, there can never be any certainty that the difficulty—and hence the need for the rule—will ever come to light if only one of the persons concerned is interviewed.

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[1] ' The Use of Sampling in Great Britain,' C. A. Moser, *Journal of the American Statistical Association*, 44, 1949.

[2] ' Sampling for the Social Survey,' P. G. Gray and T. Corlett. *Journal of the Royal Statistical Society, Series A* (*General*), Vol. CXIII, Part II, 1950.

(Note : The National Register discussed in these papers is no longer available.)

[3] ' Methods of the Survey of Consumer Finances,' *Federal Reserve Bulletin*, July 1950..

[4] 'A Pilot Survey of Incomes and Savings,' H. Lydall, *Bulletin of the Oxford Institute of Statistics*, Vol. 13, No. 9.

One argument against the use of groups of this kind has to be considered. It seems certain that the response rate will be somewhat lower where more than one person at an address has to be interviewed. One is up against the problem here of possibly having to make several calls and thus becoming a ' nuisance ', the possibility of one hostile person persuading the rest of the ' group ' against co-operation, and the difficulties of individuals having to reveal information which they do not want other members of the ' group ' to know.

To use the dwelling-unit as the unit of analysis, on the other hand, would be increasing the variances quite unnecessarily. Dwellings may contain one or more of the natural ' groups ' we have been discussing, and some of the variance of the dwelling-unit data will be due to this variation in numbers. Provided therefore that one can obtain an estimate of the total number of ' households ' which is based on a larger sample (perhaps complete enumeration) than that for which expenditure data are available, it will normally be better to use the ' household ', or other appropriate group, as the unit of analysis.

This discussion has left out of account those persons normally resident in the area who are in fact not attached to ' groups ' in any normal sense because they are living in hotels, hospitals and institutions of one kind and another. This section of the population is very often ignored for budget inquiry purposes, but must be included for the social accounts inquiry. It seems probable that it will make for ease of collection and completeness of coverage if these residential institutions are sampled separately. No problems of definition arise because we are not concerned with presenting separate accounts for certain conceptual entities. What is excluded from one sampling scheme will automatically be included in the other.

An important way of reducing sampling errors is to use some method of stratification with variable sampling fraction within strata. The variances of the items with which we are concerned seem roughly proportional to the size of the items themselves and therefore it would be useful to stratify by some variable which is highly correlated with those items—expenditure, income, saving, etc.—but in fact it is extremely difficult to find any variable which can be used for stratification which is so correlated. If one is using the rating list as a sampling frame, the rateable value of the dwelling units seems an obvious choice. But because of the division of many of the larger highly-rated dwellings into flats, or their conversion into lodging-houses, it may well not be a guide to the economic status of the inhabitants. Both the Social Survey in their work[1] and the Oxford Institute of Statistics[2] in their savings inquiry have found a low correlation between rateable value and income group. But the Oxford Institute, who did use stratification by rateable value with variable sampling fraction, found it a more efficient method than random sampling without stratification because it brought together in the lower strata fairly similar types of units, whilst leaving in the upper stratum a mixed bag with large variance but also with a correspondingly large sampling fraction. Further experiment with this method seems desirable. One might also consider the possibility of using local knowledge simply to mark off obviously high or obviously low income areas and using different sampling fractions within these areas. Or one could go a stage further, at extra expense, and use the method of double sampling; i.e., the higher income group areas could be visited prior to interview and the selected addresses rated according to probable income group by inspection from outside, into ' high ', ' medium ' or ' low ' groups. The ' high ' and ' medium ' groups could then be sampled more intensively than the ' low '. This method also requires further investigation.[1]

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[1] See Gray and Corlett, *op. cit.*, p. 26.

[2] See H. Lydall, *op. cit.*, p. 274.

The Form of the Question

It is doubtful whether information on income, expenditure and saving, can be collected in the detail required from a single group of people. This is partly a matter of psychology and partly a function of the length of the interview or the work involved in keeping a record. There is a widespread feeling that people's objection to revealing what they consider very personal information is at least overcome to a certain extent if they are asked questions about only part of their affairs. Thus people may not mind saying how much they earn if they are not asked how they spend their money, or saying how they spend their money if they are not asked how much they have got. It would be interesting to see whether there really is anything in this idea. Certainly there is a strong argument in favour of restricting the length of the interview and the number of topics dealt with in the course of one session, both because of possible effects on willingness to co-operate and because of the effect on the quality of the information obtained.

Because it is very difficult to obtain a reliable figure for personal savings, it is desirable that we should try to estimate it both through changes in asset holdings over the course of a year and as a difference between income and expenditure. The Survey Research Centre at Michigan and the Oxford Institute of Statistics have both combined the approach through asset holdings with questions about income, with a good deal of success. The combination of very general income questions with the collection of expenditure data has been quite a common practice in work on family budgets; but to estimate income with the precision required, we are likely to need much more detailed questions than are usually asked in family budget work. A further possibility is to subdivide expenditure so that no one person is asked for details of all items, but various sub-samples of the main sample are asked about different parts of total expenditure. This might, if desired, be combined with some questions about income or about saving. The main difficulty here, if the interview method of collecting the data is used, is to ensure complete coverage of all items; for although to break down expenditure in this way makes it possible to use far more detailed prompt lists for the same length of interview, one cannot hope to specify *all* items of expenditure. But since none of the questionnaires covers the whole of expenditure, there can be no catch-all final item called ' Miscellaneous ' or ' Other-specify '. This difficulty can be overcome, however, if the interviewers are well enough acquainted with all the questionnaires to be able to recognize items which fall in the ' no-mans land ' on the borders of two questionnaires and to specify all such items in detail.

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[1] For a discussion of this method of over sampling high income areas see ' Methods of the Survey of Consumer Finances '. *op. cit.*

Decisions about the attractiveness, to the respondent, of the various combinations we have outlined above and their influence on the length of the questionnaire, can only be taken after further experiment. But one important contribution which this multi-phase sampling can make to general sampling efficiency is where the variances of different items are widely different. Then the use of such multi-phase sampling can become equivalent to using different sized samples for the different items. For instance in the case of expenditure, some items—television, cars, etc.—will be bought extremely infrequently and expenditure on these in any reasonable period will have much greater variances than on regular items like milk and bread. This may be overcome by asking everybody what they have spent on the infrequent items, and only sub-samples what they have spent on certain of the frequent items. Again, the work already done by the Oxford Institute of Statistics suggests that a very large sample is needed to obtain anything like a reasonable estimate of saving—much larger than for income for instance—and it may therefore be desirable to ask questions about saving of a much greater part of the total sample than is asked about income.

The Period Covered

Another method for reducing the variances arising from the infrequency or irregularity of expenditure is to increase the length of period covered. This will also have some influence on which sections should be combined in one questionnaire. Our object is to estimate a year's total for everything. This might be arrived at by asking the same people for details for a whole year, or by asking different sets of people for details of separate months, weeks, etc., and building up a year's total from those. For some items it may in fact be easier and more accurate to go straight for a year's total. In the case of salaried workers, or people receiving dividends and interest, a year's income makes more sense than a week's; whereas for weekly wage-earners it might be extremely difficult to give a yearly figure, except at that time of year when they receive their statement of tax deductions. Expenditure for a year is clearly a rather useless concept for most items because it requires such an effort of memory. In a shorter period however, like a week or a month, the items which are bought irregularly and infrequently will have enormously greater variances than the regular items such as milk, groceries and newspapers. As an alternative, or in addition, to taking a larger sample for the infrequent items, details might be obtained for these items over a longer period—that is, to ask for expenditure on television sets and motor cars for a year, but for milk, groceries, etc. only for a week.

Since to obtain a year's total means taking account of seasonal variations, particularly in expenditure, it seems that our best procedure will be a continuing series of investigations throughout the year—e.g. the same size sample for each week or month (or possibly different sizes if one has reason to suppose that there are seasonal fluctuations in the variances), with differing periods for different items, and different size of sample for different items.

The Method of Collection

There still remains the question—how is the material to be collected ? Generally speaking there are two possible methods. In the first people would be asked to keep a record of their expenditure for a week, a month or longer period, and to record details of income or any other items we wish to obtain. The other uses a personal interview in which individuals are asked questions about their income, expenditure or saving in some preceding period. There is a widespread, intuitive view that the records are more reliable; but apart from the knowledge that the two methods give different answers little seems to have been written about their relative accuracy or about any attempts to compare the two methods. An early discussion and some interesting data are given by Charles Madge[1], but he seems to come down on the side of record keeping with little real evidence.

A feature of account books for collecting expenditure details which is often overlooked is that they rely in fact on memory, even if completed daily (which of course they often are not), and they are usually not accompanied by very full prompt lists. Even when detailed prompt lists are provided, there is no guarantee that they are read right through each time the record is completed. It thus seems that the completion of an account book will often be a matter of largely unprompted memory for a period of two or three days—or even more—and its accuracy may be appreciably less than is often assumed. Efficient prompts are however essential in the interview method, even if a period as short as a week is to be covered, otherwise quite important things may be overlooked. On the other hand, a very detailed prompt list may lead to an intolerably long interview. The possibility of breaking down the material to be collected into a number of different questionnaires appears to be the solution to this difficulty.

An interesting experiment in using the interview method to collect expenditure information, but using no prompts, was tried recently by the

[1] *Wartime Pattern of Saving and Spending*, C. Madge, Cambridge, 1943.

Social Survey when they made a survey covering only one day's expenditure for each individual interviewed. From the point of view of the quality of data obtained from individuals and the response rate, this may be quite efficient. It does seem to pose certain rather serious problems of other kinds however—for instance, the difficulty of estimating household expenditure because of the practical impossibility of interviewing all members of a household on the same day, the need for reweighting within the week because of inevitable bias in the days on which certain people can be contacted, and other estimation problems.

One advantage of the record method is that, provided it is kept honestly, the period covered is quite clearly defined at both ends. There is a real danger in the interview method of getting over-statement because the beginning of the period is for the interviewee a vague date in the past, and he has the burden of allocating expenditure either to the period under review or to the earlier period. This would not matter so much if the allocation were random, but if it is true that people like to make an impression there is every reason to suppose that there will be bias towards allocating too much expenditure to the survey period.

Records however are certainly not unbiased. They are open to the very serious objection that the keeping of the record may itself affect expenditure patterns especially if it is kept for a number of successive weeks. And perhaps most important of all the response rate with records is not very high, certainly, it appears, not as high as with the interview method. Even payment does not seem to outweigh the disadvantages, from the point of view of the record-keeper, of having the bother of keeping an account. The argument that it will all be over in half-an-hour or an hour operates strongly in favour of the interview.

Yet another factor to be taken into account in considering the relative merits of the two methods, is cost. On the face of it the interview method seems likely to be the more costly in interviewer's time, because of the necessity, if households are the basic units, of contacting all people in a household and of going through the questionnaire with each of them. But if account books are kept a personal interview with all individuals who are asked to keep them, at both the beginning and end of the period, would seem to be a minimum for efficiency. If the period covered is greater than a week intermediate visits are also desirable. This, combined with the difference in response rate, will tend to equalize cost per completed schedule.

The evidence therefore does not seem conclusive one way or the other. Whatever else may be said, the very low response rate which is obtained in household expenditure studies using account books is a considerable stumbling-block unless some means can be devised for making a very full and adequate study of the non-respondents. For this reason we are inclined to think that the interview method, if developed along appropriate lines, may well yield better and more reliable results than account books. In any case, far more has been recorded about the use of account books than about the use of the interview procedure. We have therefore sought in our first two pilot investigations to develop the interview as a means of collecting household expenditure information.

Part II. The First Pilot

Our first pilot survey among households was carried out during the month of February 1952. We decided to confine our attention to expenditure, because this was the field in which most experimental work had already been done and we were therefore able to draw on the experience of others and to form fairly clear ideas about the way certain questions should be asked and so on. This enabled us to devote rather more attention to such things as problems of organization of which we had, as yet, no experience. Our objects in the pilot might be said to be fourfold—first to try the interview method to collect details of all personal expenditure; second, to obtain some information about the variances involved; third, to discover if there were any problems peculiar to Cambridge, and to test the reaction of people in Cambridge to this kind of activity; and last to see whether it was possible to recruit suitable interviewers in sufficient numbers in Cambridge and to build up a trained nucleus of such interviewers. One might add to this list a fifth object, which was to extend our own experience and knowledge of survey organization, about which little is written, but which is of great importance for this kind of work.

We restricted this first survey to the City of Cambridge and to normal residents of dwelling houses, excluding those living in institutions. Our definition of ' normal resident ' excludes undergraduates unless their permanent home is in Cambridge, We felt that we should not try to cover expenditure by children under 15, or children still receiving full time education provided they did not have more than 10s. a week pocket money, since we believe that in most cases the money which children handle will be received from parents, and that we can pick it up at this stage. We have been criticised on the grounds that we shall underestimate expenditure on sweets and comics !

We drew a sample of dwellings from the rating list with the intention of ultimately analysing expenditure by households. A sampling fraction of 1 in 80 yielded 309 addresses, a size of sample with which we felt able to cope adequately, and still large enough to yield useful information. There was no stratification by rateable value in this pilot. Our interviewers were therefore instructed to call at the selected addresses and there interview everyone ' normally ' resident, and over 15 years of age.

We also had to group them into ' households '. Even though we had decided that the ' household ' was probably a more economically homogeneous unit for our purposes than the individual, we were still faced with the problem of choosing between several possible definitions of household.

The one we have used is:

(*a*) all members of one family related by blood, marriage, or adoption, who live together (except these members who normally make their own arrangements for meals and who then constitute a separate household)

(*b*) other people living together and contributing to a common pool for meeting the main household expenses

(*c*) people who are not in either of these categories are single person households, with the exception of resident domestic servants who are to be regarded as part of the household of the family by whom they are employed.

This has the effect of making lodgers and boarders for the most part, separate households.

The field work was carried on for four weeks from February 4th to March 2nd. The sample of addresses was divided at random between two fortnightly periods. Within those periods interviewers were free to make their calls whenever they wanted to, although they were asked to spread them as evenly as they could over the two weeks.

We wanted to obtain details of all expenditure, and in fact people were asked for details of all items during the seven days preceding the date of interview. But in an attempt to deal with the items which have the larger variances we tried to cover these for periods longer than a week. The combination of periods we finally used was in fact very similar to those which have been used by the Social Survey in some of their work. For regular items like insurance, rent, rates, licences, the date of last payment and the period covered by it were obtained; for a restricted list of what we called 'Major Durables' people were asked to remember a year's expenditure; whilst for clothing, leather goods, sports goods, etc., we covered three months. People were also asked to indicate whether the expenditure was inside or outside the county of Cambridgeshire (expenditure outside the county being, for our purposes, a transaction between the household sector and the Rest of the World). The interview ended with the collection of some personal information about the persons interviewed, age, sex, occupation and income under eight broad groups.

A word should be said about our interviewers. They were obtained by public advertisement, and with two exceptions had had no previous experience of interviewing work. After some twenty hours training on the nature and method of survey work, the group did some practice interviews before finally going into the field. They were instructed to call at the Department every week during the course of the survey to return completed forms, which were immediately checked and doubtful points cleared up.

The first thing to be considered about the results of the survey is the response rate. In terms of completed expenditure forms, that is of individuals, we achieved a response rate of 70 per cent of all individuals available for interview. In terms of dwelling units or addresses where everyone eligible completed the questionnaire it was 65.3 per cent, in terms of complete households very slightly higher—65.8 per cent. This response rate seems quite encouraging but by no means satisfactory. It is extremely difficult to compare it with the results of other surveys done elsewhere because these have rarely involved obtaining the co-operation of everyone at an address, which seems inevitably to reduce the response rate because of the difficulty of contacting everyone in the house and the possibility of one hostile person persuading other members of the household to refuse. The 70 per cent individual response rate can be compared with the average response rate of 75 per cent obtained by all interviewers for the savings questionnaire, the most difficult and most like our own in the London School of Economics experiment.[1] A more nearly comparable response rate for dwelling units is probably that obtained by the Oxford Institute of Statistics in their pilot Savings Survey,[2] when their interviewing unit was the ' spending unit '. Their sample contained a higher proportion of the higher rateable value dwellings, and since there was considerable variation in the response rate between the different rateable value groups their weighted response rate was 70 per cent, unweighted only 64 per cent. There appeared to be nothing like such variation in response between our rateable value groups.

One of the most important questions is whether we discovered anything about the non-respondents which would enable us to assume that our respondents were in fact a random sample of the population in respect of the characteristics we were investigating. The information is rather inconclusive even when as much use as possible is made of it. The 105 dwelling units where complete co-operation was not obtained (they can be further broken down into 7 where complete households were obtained, 29 where individuals co-operated and 69 complete refusals) fall into two groups—those (15 in number) for which no information about number of residents, age or relationship, is available, and the remainder where such information, and in some cases even more, has been obtained. All we know about the 15 is the rateable value, and the interviewer's rating. (Interviewers were asked to classify each address into very broad income groups, less than £400, £400—900, £900—1500 and £1500 and over. A purely subjective assessment to be made on the basis of inspection of the outside of the dwelling, and what could be seen of the furnishings, etc.). We tested the 15 for any association with either rateable value group, or interviewer's rating, but the application of χ^2 test showed no significant association. Careful perusal of interviewers' reports also indicated no particular features about this group of refusals.

The application of the χ^2 test to the other non-co-operating dwelling units also showed no significant association between rateable value and refusal, or interviewer's rating and refusal. Further tests were made for association between non-response and the total number of people living in the dwelling unit, the number of households in the dwelling unit, and the number of people over and under 15 years of age in the dwelling unit. The only significant result was obtained in the case of the number of persons under 15, where it appeared that there were fewer in the non-co-operating dwelling units. A near significant result was obtained in the case of the number of individuals over 15. This seems to suggest that we have under-representation of dwelling units with no children under 15 in our sample. This is interesting because houses with no young children will frequently contain people whose families have grown up.

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[1] ' Differences in Response Rates of Experienced and Inexperienced Interviewers.' J. Durbin and A. Stuart, *J.R.S.S., Series A. (General)*, Vol. CXIV, Part II, 1951.

[2] H. Lydall, *Op. cit.*, p. 267.

Another useful check on the randomness of the final sample of respondents may be applied by comparing any independent estimates of population characteristics which may be available, with estimates of those characteristics derived from the sample. Such estimates are available from the 1951 Census for the total population of the City of Cambridge and its age and sex composition. Our sample gives an estimated population of Cambridge of 77,623 with a sampling error of 2,463. This estimate excludes all persons resident in hotels, guest houses, institutions, and persons away from home on national service, whereas the census figure of 81,463 at April 1951, does include most of these people. The one per cent sample from the 1951 Census, on the other hand, included only 758 people resident in private households in Cambridge, which agrees fairly closely with our figure.

The information on age and sex distribution available from the Census is also in the one per cent sample tables and is itself subject to sampling error. Furthermore, it relates to the total population and not only to those resident in private households. It does appear, however, that our respondent households contain a greater proportion of children under 15 years of age than we should expect. This is in accord with our earlier suggestion about under-representation in our sample of dwellings without children under 15. Among the over-15's our sample contains almost exactly the same proportions of males and females as the Census sample, and this agreement is quite close in each of the age groups. But compared with the Census we have under-representation of both sexes in age-groups 15–24, 45–54, and over-65, with corresponding over-representation in the others—though this is less marked, and certainly not significant, in the 55–64 group. The sampling errors of both our own and the Census figures can only be roughly estimated; but it seems that the discrepancies are probably near significant at the 95 per cent level. Under-representation of the older people again accords with the apparent under-representation of households without young children, but it is less easy to account for our shortage in the 15–24 age-group. It may result from some under-representation of lodgers, who might well be young. There is little real evidence for this, but we know of at least two large houses, containing numbers of lodgers, where landladies resolutely refused access to other occupants. This creates a problem which it is difficult to solve. Where names can be obtained (e.g. from the electoral register) it may be possible to make contact by a direct approach to the lodger by name—possibly by post in the first instance.

An important point for consideration is the extent of ‘ non-contact ’ (which is included in what has hitherto been referred to as non-response), and what can be done in future surveys to reduce this undoubtedly important factor. The extent of complete failure to contact anybody at the selected addresses was very small indeed—only four, and in two of these it was discovered from neighbours that the people were away. Our interviewers were undoubtedly working under very easy conditions from this point of view. None of their addresses were very far from their homes, and they could therefore vary their times of call and were encouraged to make more than the minimum three calls. But contact with individuals presents a different picture. Out of 189 individuals who were available for interview but who did not co-operate, 50 per cent were not seen personally. Some of these were never found in, in spite of repeated calls, others left messages or notes saying they did not want to co-operate, and in other cases people who were seen said that other members of the dwelling unit would have nothing to do with the inquiry. The only way to overcome this difficulty is for interviewers to be much firmer in their insistence that they are asked to interview *all* people living at the address, even if the people concerned are only going to refuse to help. It will often be found that other people are in fact willing if they are only given the opportunity, and in any case personal contact with the interviewer enables them to hear an unbiased account of the objects and methods of the survey.

Our impressions about how our form worked, the quality of the information obtained and so on, are almost entirely subjective. But one of the great values of our close contact with the interviewers during the course of the survey was that it gave us a very good ‘ feel ’ of how the thing was going. A general criticism of the form we used was that many of the expenditure groups were too broad. This created difficulty for the interviewers in allocating doubtful items, even though they were instructed to specify these separately, but, more important, these broad groups definitely did not act as efficient prompts. A particularly bad example was the case of clothing which we had only divided between men's and women's wear, and boots and shoes.

The construction of an efficient prompt list presents another problem of achieving a proper balance between conflicting objects. It is clear that no prompt list, however long, could ever cover all the possible objects of expenditure. The list has therefore to be so devised that it will specify a number of the more common items—especially those like 'bus fares, which although common are easily overlooked—and at the same time it should suggest to people's minds objects of a similar nature which are not actually specified. For this purpose it is advantageous that the prompt list should be as detailed and comprehensive as possible. It is quite clear, for instance, that just to prompt for ‘ clothing ’ frequently fails to remind people that they bought a pair of socks or stockings a few weeks ago. On the other hand, too long a prompt list makes for a long and tedious interview. To hear an interviewer reading over long lists of items on which there has been no expenditure during the period may either dull the respondent's senses to the extent that he does not really listen and so omits items which really are relevant, or it may have the opposite effect and make him seize on any chance of saying 'Yes' to an item, even though his expenditure on it was in fact outside the period under consideration. To reduce the scope of the interview whilst retaining very detailed prompts will prevent it from becoming unduly long, but it will not overcome the discouraging effect of a long recital of irrelevant items. This is a problem which can only be solved as a result of experience and the accumulation of evidence on which items seem most readily remembered or overlooked.

The section of our form which covered the regular items of expenditure (rent, rates, insurance, licences, etc.) appeared to work very well, and people were willing to consult records. Last week's expenditure on food, drink and tobacco, postage, etc., was also quite successful, and so was the list of major durable goods. The sections which definitely did not work were housing repairs and decorations for a year, and a miscellaneous group of items (clothing, books, etc.) for three months. Part of the difficulty was undoubtedy that this period included Christmas, and interviewers reported a great deal of trouble in trying to get people to recall expenditure on Christmas presents. But in general it was too much to ask people to think back over so long a period for such a variety of commodities, particularly at the end of a rather lengthy interview. The whole form demanded too much mental agility on the part of the people being interviewed. Not only did they have to cover all their expenditure, but for many differing lengths of time.

Our interviewers reported that the length of the interview did not appear to affect people's willingness to co-operate. For the main spender in the family it would usually take over an hour to complete, but for the rest of the household usually only thirty minutes. Interviewers were instructed not to win interviews on false pretences by saying that ' it would take only a few minutes '. But it is possible of course that the response would have been higher had they been able *truthfully* to say it would take only a few minutes.

In the early stages we were rather concerned about how to ' sell ' our inquiry. We were offering people no direct reward for co-operation, and our reasons for conducting the inquiry were rather remote from their every-day life. In general however we were amazed by how little people in fact wanted to know. It was connected very vaguely in their minds with the cost of living, about which they had strong views and that was enough. It was unfortunate in a way that most people who co-operated were so readily persuaded to do so, for it gives us very little guide to ways in which our survey can be made more attractive to those at the other extreme—who were equally firm and immutable in their refusals.

PART III. THE SECOND PILOT

The problem of improving the quality of the information obtained and the possibility of making the interview easier for the respondents was uppermost in our minds in planning our second pilot expenditure study, which was in the field from June 12th to July 10th. It seemed that some solution might be found in sub-dividing total expenditure along the lines suggested in Part I of this paper, and that this might also make for a higher response rate. We were also interested in doing some experimental work on the collection of income data alongside expenditure information.

The main structure of the pilot remained the same. We drew a random sample from the rating list for the City of Cambridge but this time we decided to use some stratification by rateable value. We divided the population into two groups, dwellings of rateable value £0–30, and £31 and over. In the first group we used a sampling fraction of one in 120 which yielded 182 addresses, and in the second group, one in 25 which gave us 117, a total of 299. In addition we decided to make a sample check on a list of addresses which had previously been rejected as being non-residential, which brought the total number of addresses to be visited up to 314. We decided to use the interview method again, and the same definitions as before, so that all persons normally resident at the selected addresses would be interviewed, and grouped into ' households '.

The innovation was the type, or rather types, of form used. We had eight different types of form, four sets of expenditure questions combined with two sets of income questions. Each set of expenditure questions had certain things in common—all four contained questions on purchases of certain specified major durables (including houses) during the last year, questions on hire purchase, on insurance payments, and on rent and housing payments. The other items were split along the following lines—the first form dealt with household equipment and textiles, cleaning materials and domestic service, the second with furniture, leather goods, jewellery, sports equipment, books, newspapers, entertainment and alcoholic drink, the third contained questions on maintenance and running of vehicles, travel, fuel and clothing; and the fourth, decorations, repairs and improvements to houses, and food, drink and tobacco. The four were designed to cover all items of expenditure between them. It will be seen that, in addition to the questions common to all four forms, another difficult item, alcoholic drink, was included in two of them.

The period covered by the expenditure questions varied. For major purchases we again covered a year, and for the regular payments such as rent and insurance we collected information on how often the payments were made, and the date of last payment. For the remaining items we used a period of either four weeks or seven days, according to the frequency with which the items were likely to be bought. We did not use the three-month period at all in this pilot.

The difference between the two sets of income questions lay in the fact that in one case people were only asked to indicate into which of several broad income groups they came, while in the other they were asked detailed income questions. Employed persons were asked how often they were paid and what they were paid last time, and self-employed were asked their gross income for the latest available period. We used a prompt card for other items of regular income, and asked people the source, the amount and how frequently it was received. There was also a question asking for any casual receipts of money during the last four weeks.

The eight different types of form were allocated entirely at random to the selected addresses, and the same set of questions was asked of all persons living at one address. It can be seen from this that in effect we had a main sample consisting of eight sub-samples.

There has been no time to analyse, in any detail, the material collected during the pilot, but it may be useful to give certain preliminary results and impressions. The response rate was very much higher than in the February pilot. Of the individuals approached 82 per cent gave complete co-operation and in 77 per cent of the dwelling units everyone co-operated. This is a very encouraging result, but it is difficult to attribute the improvement to any single factor.

It is an interesting feature that the improvement in the individual response rate results almost entirely from an increase in the proportion of dwellings in which *everybody* co-operated. There was very little change in the proportion of dwellings in which some individuals or some households provided information while others did not. The actual figures were:

	Pilot 1 %	Pilot 2 %
Dwellings with co-operation from everyone	65.4	76.5
Incomplete dwellings with co-operating households	2.3	2.0
Incomplete dwellings with co-operating individuals	9.6	8.1
Dwellings with refusal from everyone	22.8	13.4

There was a slight reduction in the amount of non-contact. Someone was seen at every dwelling unit visited, except one empty one, and one or two which turned out to be non-residential. Our interviewers were instructed to make special efforts to try to see everyone personally. Out of the 130 individuals who refused, 57 per cent were seen personally, compared with 50 per cent in the first pilot, 17.6 per cent gave refusals through other people or through notes, and 25.4 per cent were genuine non-contacts in the sense that the interviewer was not allowed to see them, or was unable to see them after repeated calls.

Two other factors which may have had some influence on the response rate, were stressed by interviewers. The first was the much shorter interview, they found it particularly helpful with people who were at all hesitant to be able to assure them that the interview would only take 20 minutes or half an hour. Interviewers were also armed with preliminary results of the previous pilot to give to people; they found these very useful both in interesting respondents in the survey and in convincing them that the survey really was confidential. The argument that to be asked about only part of one's expenditure is less prying than to be asked for all does not seem to be a very potent one in winning co-operation. At least, our interviewers do not seem to have made much use of it. Undoubtedly the time of year had something to do with willingness of people to talk. Light evenings made them less suspicious, and warm weather more willing to discuss on the doorstep. It seems unlikely that the increased experience of the interviewers who had worked for us in February had very much to do with the higher response rate, because at a cursory glance it appears that new interviewers recruited and trained for the second pilot did just as well.

It is interesting to note that, although they had no instructions on this point, the interviewers all avoided mentioning the fact that they were going to ask questions about income at the beginning of the interview. But at the same time they were all quite convinced that no one seemed surprised, or felt that an interview had been obtained under false pretences, when this part of the questionnaire was reached. We have analysed the response to the detailed income questions among individuals who were willing to provide expenditure information, in dwellings where everyone co-operated. Of these, 86.5 per cent gave complete income information (so far as we are able to judge), 11.0 per cent were willing to give some information or a broad income group, 2.5 per cent gave no information at all. There was no significant variation between strata in this respect. Of the people who were only asked to give a broad income group, 4.0 per cent of those who had given expenditure information refused to answer the income question.

Until the figures have been analysed and comparisons made we can only give general impressions of how the expenditure part of the form worked, on the basis of interviewers' reports. They found that in general people were not confused by being asked for only certain items of expenditure, but were often disappointed at having so little to tell. We have the impression that four weeks is a more manageable period for memory than is three months, except for very large items of expenditure. It may well be however that we have been wasting information, in the sense that we could have collected more from each individual without reducing the quality. It is necessary to achieve a balance between the improvement of quality by asking each individual as little as possible, and the reduction of sampling errors by asking as many people as possible about particular items. We feel at the moment that we may have swung too far in the direction of reducing the number of questions; but this will require further study when a more detailed analysis of the forms has been made.

PART IV. ESTIMATION PROBLEMS[1]

The interpretation of the results of these surveys presents the difficulty—common to most studies of personal income or expenditures—that the distributions with which we are concerned are extremely skew. If, for instance, we consider total household expenditure last week (which was obtained in the first pilot study, but not in the second) its distribution for the households in dwellings for which we have complete information is as shown in Fig. 1. This is obviously very skew indeed; in fact the arithmetic mean is £10.08 which as can be seen from the diagram is much greater than the mode. The same distribution has also been plotted on arithmetic probability paper (Fig. 5). If the distribution were approximately normal, the points of this graph would lie roughly along a straight line. Clearly they do no such thing.

If we consider last week's expenditure on particular groups of commodities (e.g. food, clothing, drink, tobacco, travel) the situation is in many respects worse; not only is the distribution very skew, but for most commodity groups there is a substantial number of households with zero expenditure.

For some of the commodity groups for which we have obtained expenditure over a longer period, the distribution of these expenditures appears somewhat less skew and contains fewer zeros; but in the majority of cases the skewness remains considerable, and there is no reason to suppose that in some cases (e.g. the major durables group) there would be a substantial reduction in skewness even if the period could be lengthened indefinitely—though the zeros might perhaps be entirely eliminated. Any narrowing of the commodity groups will, of course, make it harder (or impossible) to eliminate zeros even by lengthening the period.

The difficulty arising from this skewness is that we cannot proceed directly to apply normal probability theory. Also although the sample mean will always be an unbiased estimate of the population mean (and hence lead to an unbiased estimate of the population aggregate which we require) further investigation of the efficiency of this method is desirable. Another question which arises from this skewness is whether the usual formula for sampling error can be relied upon for a good indication of the confidence limits of the mean.

We are thus led to seek some other variable which is more nearly normally distributed than the original total expenditure variable. From a mathematical statistical point of view, the simplest procedure for handling material which is distributed like Fig. 1, is to take logarithms. The resulting distribution is shown in Fig. 2. This is much more symmetrical and appears quite possibly to have been obtained by taking a sample from a normal population. The original distribution has also been plotted on logarithmic probability paper (Fig. 6). This would give points lying on a straight line if the original distribution were log-normal. The goodness of the fit may again be judged from the graph. The change of slope at the right hand end is a sampling effect, due to the fact that it is very improbable that the highest values would be included in any particular sample. The diagrams may be compared with the corresponding charts for an artificially constructed sample from a log-normal population. The distribution of this sample is shown as a histogram in Fig. 4, and has also been included in the ' probability ' diagrams at Figs. 5 and 6. Besides normalising the distribution a logarithmic transformation also has a tendency to stabilise the variances. This fact becomes important when we have to compare two or more populations.

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[1] Much of this part of the paper is due to J. Aitchison, who is in course of preparing a separate paper on this subject. The results presented here should, for the time being, be regarded as tentative.

Log-normal distributions

If x is the original variate and we assume that $y(=\log_e x)$ is in fact normally distributed with mean μ and variance σ^2, then the mean value of x is $\alpha = e^{\mu+\frac{1}{2}\sigma^2}$ and the variance of x is $\beta^2 = e^{2\mu+\sigma^2}(e^{\sigma^2}-1)$. If the sample is $x_1, x_2, \ldots x_n$ then the usual unbiased estimates of α and β^2 are respectively

$$\bar{x} = \frac{1}{n}\sum_{i=1}^{n} x_i$$

$$v^2 = \frac{1}{n-1}\sum_{i=1}^{n}(x_i-\bar{x})^2$$

Finney[1] has given estimates for α and β^2 which are both unbiased and most efficient and so provide measures of the efficiency of $\bar{x}$ and v. These estimates are

$$a = e^{\bar{y}}g_n(\tfrac{1}{2}s^2)$$

$$b = e^{2\bar{y}}[g_n(2s^2)-g_n(\frac{n-2}{n-1}s^2)]$$

where

$$g_n(t) = 1+\frac{n-1}{n}t+\frac{(n-1)^3}{2\,!\,n^2(n+1)}t^2+\frac{(n-1)^5}{3\,!\,n^3(n+1)(n+3)}t^3+\ldots,$$

and

$$\bar{y} = \frac{1}{n}\sum_{i=1}^{n} y_i$$

$$s^2 = \frac{1}{n-1}\sum(y_i-\bar{y})^2$$

with $y_i = \log_e x_i$ for $i = 1, 2, \ldots n$.

The series $g_n(t)$ is not very suitable for computation and Finney gives an asymptotic form for large n, viz.

$$g_n(t) \sim e^t[1-\frac{t(t+1)}{n}+\frac{t^2(3t^2+22t+21)}{6n^2}+\ldots]$$

[1] ' On the distribution of a variate whose logarithm is normally distributed,' by D. J. Finney. *Supplement to the Journal of the Royal Statistical Society*, Vol. VII, No. 2, 1941.

from which good approximations to a (for $n > 50$) and to b (for $n > 100$) may be obtained.

It is found that for commonly occurring values of σ^2 there is remarkably little loss of efficiency in using $\bar{x}$ rather than a, but there is considerable loss when v^2 is used instead of b^2. For example we have carried out the computations on our February sample of 227 households (in complete dwellings) and have the following results.

$\bar{x} = 10.080$

$a = 10.088$

$v^2 = 66.0$

$b^2 = 67.5$

The efficiencies of $\bar{x}$ and v^2 are 96 per cent and 39 per cent respectively and, although there seems little difference in this particular case between the values of v^2 and b^2, we feel that it is advisable to use b^2 for estimating variance. The use of $\bar{x}$ rather than a is to be recommended because of the simple additive properties of arithmetic means. If considerable use were to be made of $g_n(t)$ it might be advisable to tabulate this function over the likely ranges of n and t.

To obtain confidence intervals for α it seems simplest to make use of the fact that for large samples $\bar{x}$ will be distributed approximately normally with mean α and variance $\frac{\beta^2}{n}$. We could then use $(\bar{x} - 1.96 \frac{b}{\sqrt{n}}, x + 1.96 \frac{b}{\sqrt{n}})$ as an approximate 95 per cent confidence interval for α. For the February sample this gives

$$9.011 \leq \alpha \leq 11.149$$

Expenditure Per Head

An alternative proposal which has been put forward for overcoming the extreme skewness of ' expenditure per household ' is to use ' expenditure per head per household ' (i.e. household expenditure ÷ no. of persons in household). This is, of course, not the same as ' individual expenditure ', the concept which was rejected in Part I. At first sight ' expenditure per head ' does appear somewhat more symmetrical than ' expenditure per household ', since it rises somewhat less steeply on the left; but, in our first pilot at least, expenditure per head *on all commodities* has so long a tail to the right as to nullify this apparent improvement. This may be seen from the histogram at Fig. 3, and from the two curves which have been plotted on probability paper (Figs. 5 and 6). In fact, it looks as though, in our sample, total expenditure per head is more nearly log-normal than is expenditure per household. It may nevertheless be true that for certain basic commodity groups, such as food—and still more, for particular foodstuffs—expenditure per head is fairly symmetrically distributed. In that case it would be reasonable to apply normal theory using the ratio method of estimation.[1]

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[1] See, for example, Sampling Methods for Censuses and Surveys, by F. Yates, sections 6.9 et. seq. (Griffin, London, 1949).

The Treatment of Zeros

Reference has already been made to the problem of the treatment of the zero expenditures which occur when one is concerned with expenditure on only a limited group of commodities in a given period (or even with total expenditure if the period is short enough, as in the one-day survey). It appears that we then have a distribution consisting of a number of zeros (depending on the commodity and the period) and a number of expenditures greater than zero which are again distributed approximately log-normally. We may therefore suppose that, for the given commodity and period, the population consists of a proportion p of non-spenders and the remainder (spenders) whose expenditure is log-normally distributed as before. Then the true mean of the population is

$$\gamma = (1-p)e^{\mu+\frac{1}{2}\sigma^2}$$

An unbiased estimator of this, which is more efficient than the arithmetic mean of the sample values, is :

$$c = (1-\frac{r}{n})e^{\bar{y}}g_{(n-r)}(\tfrac{1}{2}s^2)$$

where n is the sample size, r the number of non-spenders, $\bar{y}$ the sample mean of the transformed non-zero expenditures, and s^2 their variance. $\bar{y}$ and s^2 are now dependent on r. An indication of the accuracy of this estimate can be obtained from its variance which is given by :

$$\text{var }[c] = \frac{e^{2\mu+\sigma^2}}{n}\ [p(1-p)+\tfrac{1}{2}(1-p)(\sigma^4+2\sigma^2)+\frac{1}{8n}(\sigma^8+4\sigma^6)]$$

for large n and p appreciably less than 1. As in Finney's theory it is found that the arithmetic mean of the untransformed sample is almost as efficient as c and has the advantage of simplicity. A theory similar to that of Finney is possible for estimating the variance of the population, in this case estimating the variance directly from the untransformed sample involves considerable loss of efficiency.

A next step in our investigation will be the application of these methods of estimation to the information obtained in our pilot surveys. The treatment of zeros will be particularly appropriate to Pilot II where we only have expenditure on particular groups of commodities, and have to build up our estimate of the total by way of these separate groups.

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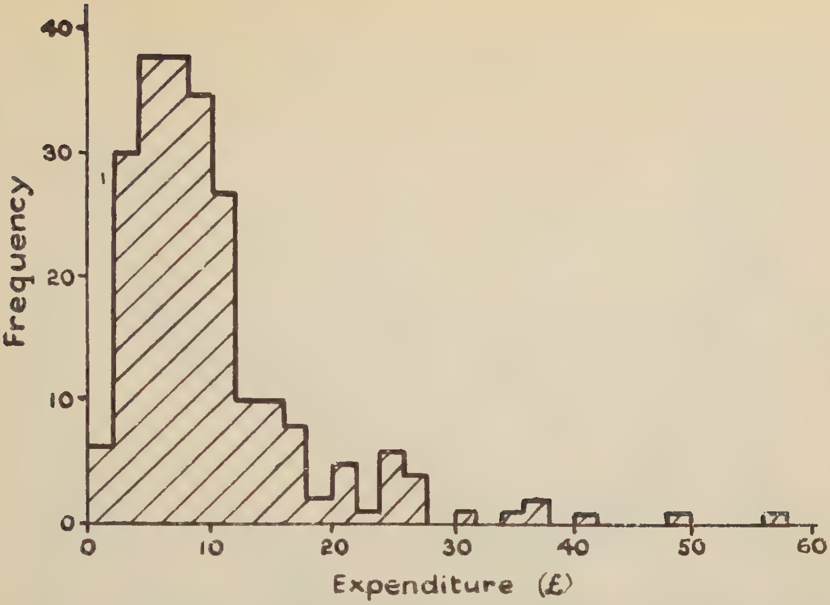

Fig. 1. Frequency Distribution of total household Expenditure.

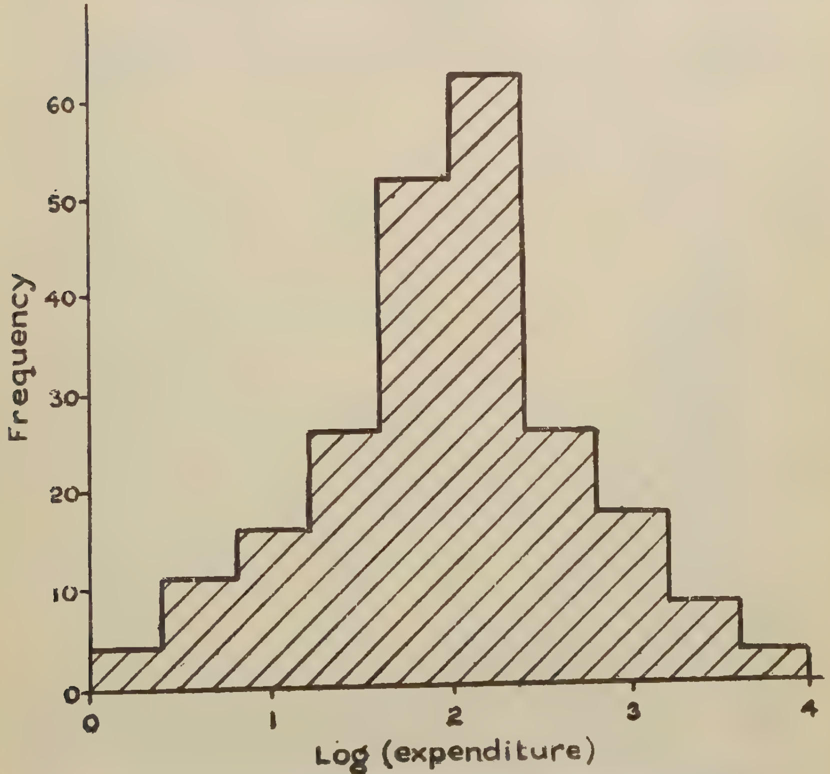

Fig. 2. Frequency Distribution of Log (Household Expenditure).

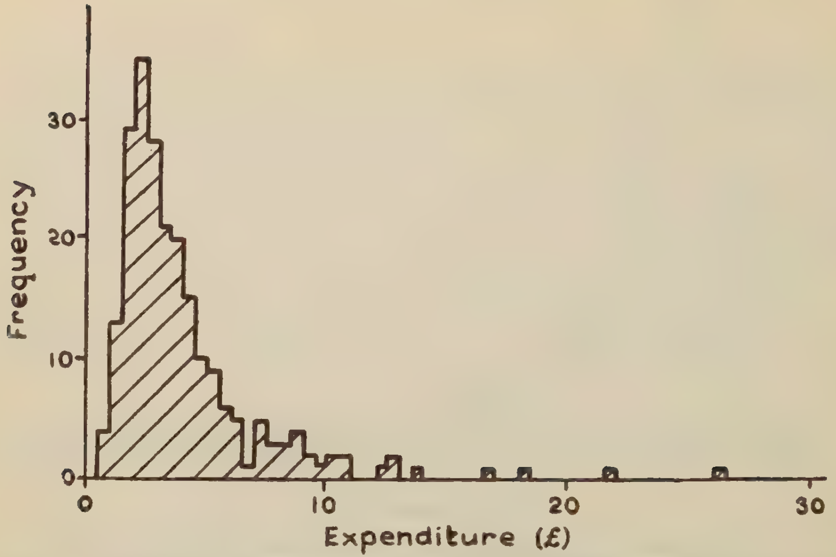

FIG. 3. FREQUENCY DISTRIBUTION OF TOTAL EXPENDITURE PER HEAD PER HOUSEHOLD.

Note.—This graph excludes one household for which total expenditure per head per household is £57.

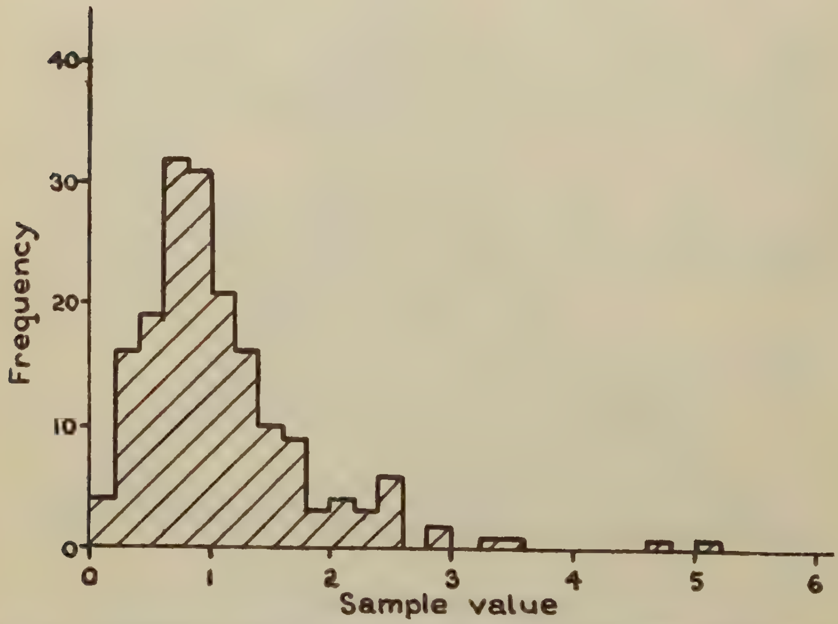

FIG. 4. FREQUENCY DISTRIBUTION OF ARTIFICIAL LOG-NORMAL SAMPLE.

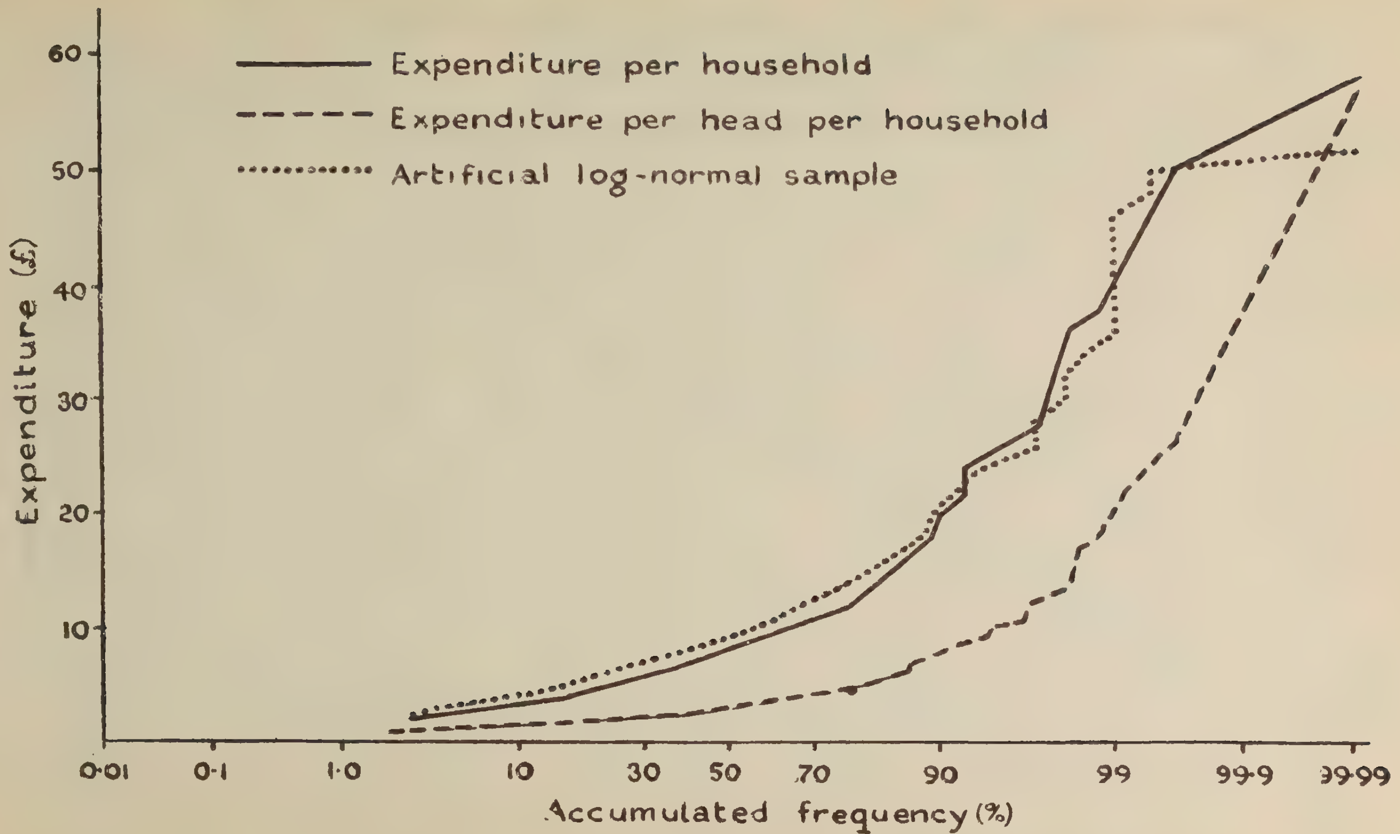

Fig. 5. Accumulated Frequency Distributions on Arithmetic Probability Paper.

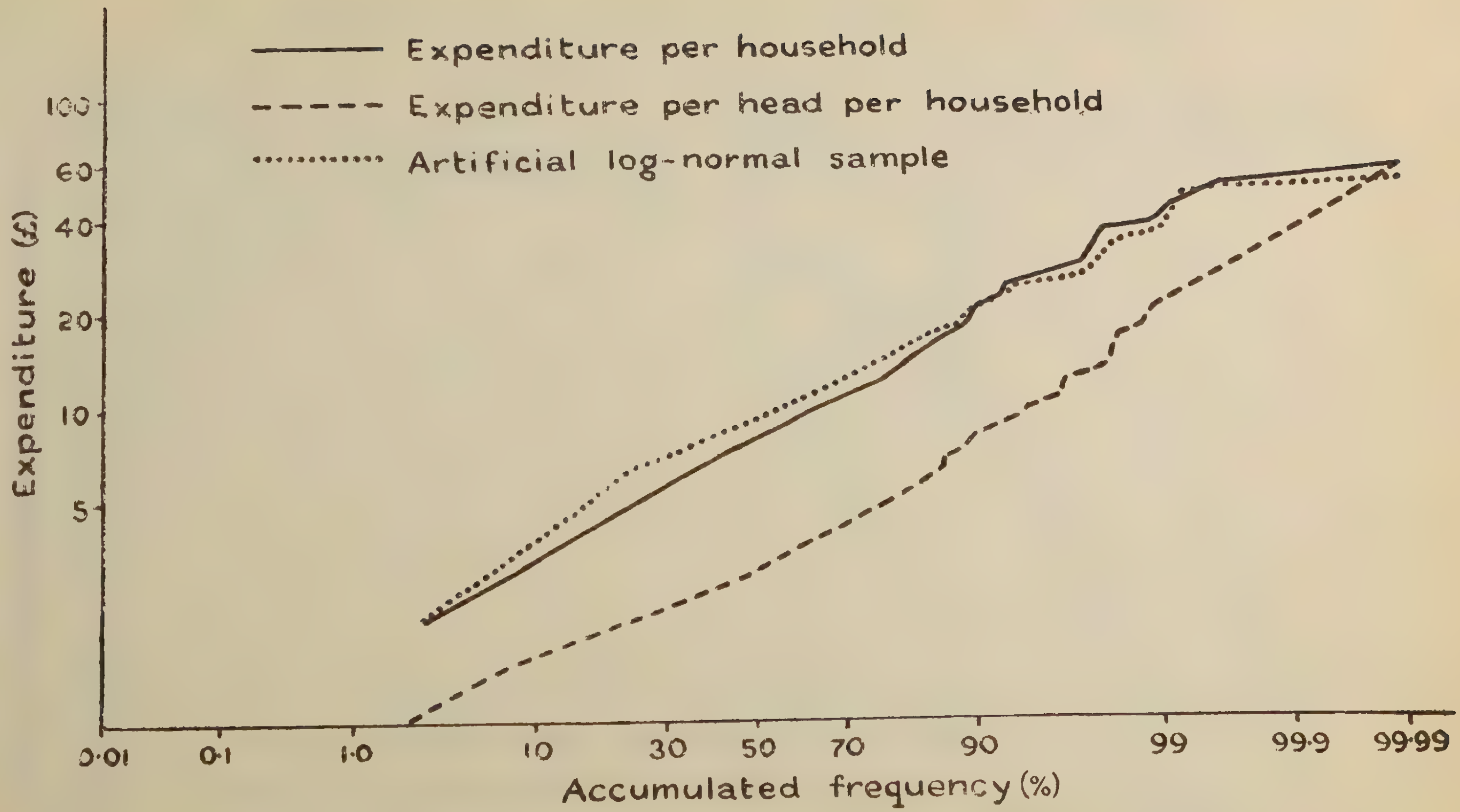

Fig. 6. Accumulated Frequency Distributions on Logarthmic Probability Paper.

THE 30 PER CENT LIQUIDITY RATIO

We published as Comment XIII in the Symposium on Monetary Policy (BULLETIN, *August* 1952) *a statistical analysis by Dr. E. Nevin of banks' liquid asset ratios from* 1925 *to* 1939. *The following notes are a comment on this analysis by Raymond Frost and a reply by Dr. Nevin.*

The Editor.

The purpose of this article is to discuss the analysis by Dr. Nevin which appeared in the August issue of the BULLETIN, of the evidence for the existence of a definite convention governing the proportion which the banks' liquid assets bear to their total deposits. His article followed a good deal of discussion in the BULLETIN and elsewhere of the part played in the government's monetary policy by the funding of part of the banks' liquid asset holdings. The assumption that the banks worked on a conventional minimum liquidity ratio of 30 per cent played an important part in devising the measures to restrict credit which were undertaken by the government in November 1951. The evidence of the past seemed to support the view that 30 per cent was the traditional ratio. But on a closer inspection of the evidence Dr. Nevin concluded that it does not support the 30 per cent convention, nor indeed does it support the belief that there is any definite relationship between the banks liquid and illiquid assets.

Before going on to criticise Dr. Nevin's presentation of the evidence, which is in my view misleading, it is first of all necessary to define what is meant by the banks' liquidity ratio. Dr. Nevin's definition is different from that which has been generally accepted in current discussion of the government's monetary policy. The ratio is usually calculated on the aggregate statistics for the London Clearing Banks taken together. The method is to include in the total of liquid assets, the following items :—

1. Cash in hand and balances with the Bank of England.
2. Money at call at short notice.
3. Bills discounted.

But there is one marginal item which is liquid in the sense that it represents the command over cash within a very short period, but which does not contribute to the liquidity of the banking system as a whole. This item is that of cheques in course of collection. Looking at the system as a whole it only represents a time lag in clearing payments between banks and it should in consequence be regarded as a paper figure which inflates the total of deposits and which should not be included in the liquid assets of all the clearing banks taken together.

But Dr. Nevin is not looking at the aggregate figures; he is considering the assets of each of the Clearing Banks individually. And he concludes, quite logically, that the item of cheques in course of collection which appears in the balance sheet of an individual bank, should be included amongst that bank's liquid assets. But by including this item in his definition of liquid assets he arrives at a set of liquidity ratios which are, generally speaking, larger than the ratio calculated on the aggregate statistics, and excluding from the amount of liquid assets the total of cheques in course of collection. It is not surprising, therefore, that Dr. Nevin finds that over the period 1925–39 which is covered by his analysis, the liquidity ratios of individual banks vary on average between 34 and 37 per cent. If one deflates this by deducting cheques in course of collection from liquid assets, one gets a ratio which is rather closer to 30 per cent, and one could deflate it even further by deducting this item from liquid assets and adding it to total deposits (which would be permissible when dealing with aggregate figures). By defining liquid assets in the way he does, Dr. Nevin is really removing any common ground that might exist between himself and others who are interested in this problem.

What has led Dr. Nevin to analyse the figures for individual banks is no doubt the assumption that the 30 per cent ratio is a rule whereby bankers should conduct their investment policy. In this he is mistaken. It is a rule, not for bankers, but for the monetary authorities. The liquid position of the banking system as a whole shows them the sort of reaction they may expect to measures of monetary policy. Dr. Nevin's painstaking analysis has thrown up some interesting information about the notions which different banks have about the distribution of their assets. But their divergency of practice is no evidence against the validity of the 30 per cent ratio as a guide to the authorities. The authorities do not care whether Martins Bank or the Midland or Barclay's reduce the amount of their risk assets in response to measures of credit restriction; they are only concerned that the total amount of risk assets should be reduced. And it would appear from the behaviour of the aggregate figures in the past that if the liquidity ratio of the system as a whole is at or under 30 per cent, further pressure on the banks' liquidity will bring about the desired result. Consequently, Dr. Nevin's analysis of the figures for individual banks, which is his chief contribution to the discussion, is not at all relevant.

Further criticism would appear to be redundant. But I would, however, like to criticise Dr. Nevin's method of analysing banking statistics in isolation without regard to the many influences which affect the figures.

1. *Influences within the system which should be taken into account when interpreting the figures.*

The first of these is, of course, changes in the level of bank rate. In the period covered by Dr. Nevin's analysis, such changes are important in the years 1925–32, after which bank rate remained at the same level of 2 per cent until 1939. Between 1925 and 1932, bank rate was the only important weapon of economic policy. Consequently, to discuss changes in bank assets without taking account of bank rate is like criticising Hamlet without taking account of the Prince of Denmark. And by ignoring the relation of changes in bank rate to changes in the banks' liquidity ratios, Dr. Nevin includes in his evidence against the validity of the 30 per cent convention cases which are not at all relevant to the discussion. But it is necessary to look at Dr. Nevin's technique more closely. He takes from the records of the Clearing Banks, the occasions when an increase or decrease in liquid assets took place, and then notes the cases when such changes resulted in an increase or decrease in advances and investments, or in their remaining the same. He finds that the occasions on which a decrease in liquid assets was followed by an *expansion* of other assets outnumber the cases when it was followed by a *contraction*. And he concludes from this that there is no adequate evidence for the existence of a fixed relationship between liquid and other banking assets. But he includes in his ' cases ' those occasions when a decrease in liquid assets was unaccompanied by an increase in bank rate, or, alternatively, which took place at a time when bank rate was low and credit was easy. It is these cases which are not relevant to a discussion of the validity of the 30 per cent convention as a basis for current bank rate policy. For when bank rate is low and credit is easy, bankers can with impunity allow their liquid assets to run down, and correspondingly increase the proportion of their advances and investments. When the authorities are pursuing a neutral or relaxed monetary policy, the money market and the banking system can be assured of cheap and ready access to liquid funds in case of need, and they know they are running small risk in increasing the amount of their relatively frozen commitments. Consequently, Dr. Nevin's evidence that a reduction of liquid assets has not generally led to a contraction of other assets is not convincing, to the extent that he includes cases of a reduction of liquid assets which take place at times of easy credit policy. I would call irrelevant every one of his cases which are taken from the years 1932 to 1939.[1]

The problem of a liquidity ratio *as such* has, in a sense, been created by economists during the last few years. To confine one's attention to it, irrespective of bank rate changes, is irrelevant in the period up to 1932. During those years there was no liquid asset problem. There was only a traditional structure of bank assets to which bankers became accustomed by long habit, and to which nobody paid a great deal of attention. The problem has only arisen since the government tried, in November, 1951, to reinstate a pre-war method of economic control—the method of bank rate. In order to make it effective they felt they should at the same time re-establish the pre-war asset structure of the banking system upon which bank rate had worked to some purpose in the past, and which was set and turned ready for bank rate to play upon it. By 1951 the asset structure had greatly changed and there was good reason to suppose that bank rate would not work so effectively upon the new shape of things. The great increase in the proportion of the banks' liquid assets was expected to blunt the edge of the bank rate weapon. The banks would be able to let the full effect of credit restriction fall in the first place upon their liquid assets, and continue to expand their advances and investments without, for some time, running the risk of having insufficient liquid assets to meet their cash obligations. It would have been a long and expensive business to rely on the operation of bank rate alone to sweat down the banks' liquid assets to a level at which they would begin to feel cautious about expanding their ' risk ' assets. And so the authorities re-created, consciously and artificially, the traditional circumstances in which the total of advances and investments had shown itself to be directly sensitive to the operation of bank rate. The 30 per cent ratio was only relevant because it appeared to be a rough guide or rule of thumb indicating the asset structure in which the total of advances and investments was sensitive to bank rate. Consequently, in examining the evidence of the years to 1932, the relationship of liquid to illiquid assets is only significant when considered in relation to bank rate.

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[1] September 1931 was the last occasion on which bank rate was put up to a penal level. In that month it went up to 6, reducing to 2 at the end of June 1932, where it stayed until 1939.

Bank rate is a palpable phenomenon, working within the banking system, that affects the structure of bank assets. But there are less tangible but no less important events which also affect the figures and which Dr. Nevin has not taken into account. Banking statistics, looked at in the abstract, are essentially unrevealing. The behaviour of bankers and other dealers in the markets for capital and credit is determined to a considerable extent by expectations, or, as I should prefer to follow Marshall in saying, by hope and by fear. The result is that banking figures at the end of any particular month do not necessarily show the influences that are currently at work. What they show is what people thought yesterday would happen to-day (or, as it was to them, to-morrow). It is consequently important to make allowance for time lags when investigating the cause of a change in bank assets. Dr. Nevin has not done this—he only considers simultaneous changes. It may take several months for a policy of credit restriction to be reflected in the figures.[1] If a policy of restriction is undertaken in a given month, it is quite likely that for the next few months the amount of the bank's risk assets will continue to increase. The reason is that some borrowers will continue to take up their undrawn overdraft facilities at what may be a faster rate than that at which the banks are calling in loans to other borrowers. Also, even at a time of credit restriction the banks will continue to increase their loans to their more credit-worthy clients, if they require money, while putting the screw on more doubtful borrowers. And calling in an overdraft is not a thing that can be decided to-day and put into effect to-morrow. It is not only fair, but prudent, to let the customer have a few months' grace in order not to force him to sell the assets on which the overdraft is secured at a knock-down price. And there may be analogous delays in reducing investments. A bank may not be able to sell investments at any given moment without a prohibitive loss, and if none of its holdings are maturing it will hold on. It is probably for this reason that the banks have chosen to cut down advances rather than investments during the past year—insofar as the choice was open to them—even though advances are more profitable. All this takes time—time for which Dr. Nevin has made no allowance whatever. For he only considers the occasions on which there is a simultaneous change in liquid and illiquid assets.

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[1] In November 1919, the authorities became alarmed at the rapid extension of bank deposits, and they raised bank rate from 5 to 6. During the next 5 months the total of advances and investments rose by £161 m., and total deposits increased by £109 m., or an annual rate of increase of something like 12 per cent. Bank rate was then raised to 7 per cent in April 1920, and liquidity ratio of the system was driven down from something over 30 per cent to 26.4 per cent by May 1920. In that month the total of advances and investments began to decline. Total deposits continued to increase until January 1921. Bank rate was not reduced until April 1921. To sum up, it had taken 6 months from the time that bank rate was first increased, to halt the expansion in advances and investments, and 14 months to bring down the volume of total deposits.

2. *Influences outside the banking system which should be taken into account when interpreting the figures.*

The banking system is intimately bound up with the whole of the rest of the economy. It has assets in the money market, in the market for long-term securities, in every sector of domestic industry and trade; assets in transit across the world, and in overseas markets. Events taking place in any part of the world economy will inevitably influence the structure and distribution of bank assets. The influence may be direct or indirect. It will be direct insofar as bank assets must to some extent passively reflect events that take place in the broad context of the world economy. If a customer wishes to repay his overdraft, a banker cannot make him leave it outstanding. If a sufficient number of customers wish to repay their overdrafts irrespective of the wishes of the banks, this will be reflected in a significant change in the amount of advances. One cannot say this has anything to do with liquidity ratios, with monetary policy or with the activities of bankers. All they do is sit back, let the money come in and try to lend it elsewhere (unless, of course, there is an equivalent cancellation of credit by the monetary authorities).[1]

And there is also very little a banker can do about the effect on the amount of his advances of his customers' failure and bankruptcy. But there are also occasions when events outside the banking system—and they may be political as well as economic events—will affect bank assets indirectly, through the reactions of bankers of those events. When business activity is falling, or when it is expected to fall, bankers will be more cautious about extending their advances. They will try to call in their more doubtful loans, and raise their standards when assessing new applications. And they may behave in the same way when the political scene is unsettled at home or abroad. Also, in common with other investors in gilt edged securities, the banks' attitude to investment in gilt edged is strongly influenced by political events.

Dr. Nevin takes no account of influences working from outside the banking system—except at one point where he reinforces his arguments by bringing them in.[1]

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[1] It is pretty clear that a good deal of the reduction in bank advances which has taken place since the start of the new monetary policy last November, has resulted from the autonomous actions of borrowers who wish to repay their overdrafts, rather than from a deliberate restriction of credit by bankers.

I do not intend to make much of the further criticism that Dr. Nevin does not weight his cases for the extent and duration of the changes to which they relate. One cannot really compare a change of a few thousands over a month with the change of millions over several months. And this is particularly important when dealing with what is, after all, a rough and ready convention. The passivity of the banking system to economic events over which it has no control must prevent this convention from being anything more than a guide to the way in which bankers will re-arrange their assets. But though this flaw in Dr. Nevin's technique is important in itself, it is in another sense secondary, arising as it does out of his general approach to the subject. He is treating as purely mathematical entities a set of figures which are only meaningful when considered as representing the result of a set of events taking place in a real world, which because it is real, is also complicated and uncertain in its behaviour. No amount of meticulous accuracy in dealing with the figures can make up for a failure to take these real influences into account.

RAYMOND FROST.

London.

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[1] He finds that in 3 cases out of 4 in which liquid assets fell below average for the bank concerned, there was an expansion rather than a contraction in other assets. Of the remaining cases 16 involved one of the big five. Most of these occurred between 1929–32, and then, according to Dr. Nevin, only because the period was one in which every financial institution in the world was desperately seeking to liquidate its asets. But surely some of the occasions when a contraction in liquid assets was accompanied by an expansion of other assets were times when economic prospects were comparatively favourable. Dr. Nevin finds no space to comment on these.

A REJOINDER

May I begin by expressing my appreciation of Mr. Frost's courteous and thoughtful comments ? He has, I think, touched on some extremely important factors which are very relevant to any discussion of monetary policy. But I do not think that his criticisms become ' redundant ' if one accepts his assertion—which many would not accept—that the liquidity ratio is a guide to the monetary authorities rather than to bankers. It could guide the authorities if, and only if, the banks adopted some recognisable and reasonably consistent policy because of changes in the ratio, and it was precisely this with which my analysis was concerned. Whoever is guided by the ratio, it comes back to the banks in the end; one cannot obtain guidance from a ratio unless that ratio has some special significance for the banking system. And I am at a loss to know how the clearing banks collectively can pursue a policy which none of them are pursuing individually; yet this is the implication of the statement that the figures for individual banks are irrelevant. The particular ratio of 30 per cent might not be the critical one for each separate bank, of course; but no one following my analysis could imagine that I thought it was, since the individual returns were analysed according as the liquidity ratio (i) was falling or rising and (ii) fell below the average for bank, as well as by the 30 per cent hypothesis.

As I see it, Mr. Frost makes two main points—that liquid-asset ratios must be looked at in conjunction with changes in other factors, especially bank rate, and that adjustments to risk assets cannot be made without a considerable time-lag for which I make no allowance. Before commenting on these, however, perhaps I may say something about my definition of the term ' liquid assets '. It is agreed, I think, that when one looks at the return of an individual bank, it is arguable whether transit items are likely to be regarded as liquid assets for the purposes of policy or not.[1] There is something to be said on both sides. But it is surely rather unreasonable to say that by including transit items I am ' removing any common ground that might exist ' between others interested in the subject and myself. As Mr. Frost himself indicates, the only difference which will be introduced is that a more or less constant margin will exist between the ratios calculated on the two definitions. And the whole point of my analysis was not that a 30 per cent ratio did not exist whereas, say, a 33 per cent ratio did—in which case the question of definition would be vital—but that *no really constant ratio whatever* can accurately be said to exist for the several banks, either in common or individually. The absolute level of ratios is of less importance than their constancy.

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[1] I am at least in good company in including transit items in liquid assets, since this procedure is followed by Mr. W. F. Crick in his article' The role of statistics in monetary affairs ', *The Manchester School*, 1938, p. 129 ; and a recent number of *The Banker* records that ' some bankers . . . are including cheques in transit . . . in their computations of liquid assets ' (December 1951, p. 349). I would observe in passing that it is surprising to find that Mr. Frost proposes to deduct transit items from liquid assets and then *add* them to deposits. This is a curious saying ; if such items do not add to total liquidity neither, surely, do they add to total liabilities.

Turning to the main criticisms, the first point is that I have ignored exogenous factors, principally bank rate. It is argued, indeed, that the liquidity ratio ' is only significant when considered in relation to bank rate ', and that occasions when bank rate was unchanged ' are not relevant to a discussion of the validity of the 30 per cent convention as a basis for current bank rate policy '. I would point out that I was not discussing anything as a basis for bank rate policy; I was discussing the theory of the liquidity ratio put forward in the past and currently by various authorities. Nothing will be found in any of these authorities to suggest that this ratio is important only when taken in conjunction with changes in bank rate, as Mr. Frost suggests. In the discussions of the question in Sayers,[1] Crick,[2] Paish,[3] Manning Dacey,[4] *The Economist*,[5] *The Banker*,[6] and the testimony of bankers before the MacMillan Committee,[7] nowhere is it even hinted that the significance of the ratio of liquid assets is dependent on the level of, or changes in, bank rate. The version of the convention which Mr. Frost now advances may be a perfectly good one,[8] but it is certainly a new one, and therefore could hardly be tested in my previous discussion. If I may repay the compliment, it is surely Mr. Frost who is removing common ground with a vengeance by arguing that the secondary ratio is necessarily connected with changes in bank rate. It is this unusual interpretation which forces him into the curious statement that the liquidity-ratio problem has been ' created by economists during the last few years '; this statement may be contrasted with, for example, the statement of *The Banker* that

> it was not until the Treasury Bill ' famines ' of the thirties that the so-called ' 30 per cent ' principle came to be discussed and was elevated by some to the rank of a theory.[9]

This point is, I think, crucial. If the argument is simply that the volume of liquid assets is *one of the many* factors influencing bankers' decisions with regard to risk assets, then neither I nor, one imagines, anyone else will quarrel with it—it is indeed implicit in the conclusions of my article. But that is not the real argument at all; the argument I was examining is that there exists a conventional secondary ratio, of a rather more rough and

[1] *Modern Banking* (Oxford University Press, 1938), II, p. 40.

[2] *Loc. cit.*, pp. 129–30.

[3] ' British floating debt policy,' *The Post-War Financial Problem* (MacMillan, 1950), p.230.

[4] *The British Banking Mechanism* (Hutchinson, 1951), IX, p. 93.

[5] November 17th, 1951, p. 1220.

[6] December 1951, pp. 349–50 ; January 1952, pp. 15–16.

[7] See the references cited by Paish, *op. cit.*

[8] Although one is tempted to ask how it is reconciled with this earlier statement that ' in the 1920's periods of high bank rate and narrow liquidity ratios had a surprisingly small effect on the volume of advances.' (This Bulletin, August 1952, p. 264).

[9] January, 1952, p. 15. For evidence of the widespread discussion in pre-war days of this secondary ratio issue see, *inter alia*, *Midland Bank Review*, March–April 1934, pp. 6–7, *The Economist* October 6th, 1934, pp. 638–9, Manning Dacey, ' Cheap money technique ', *Lloyds Bank Review*, January 1947, pp. 52–3, Crick, *loc. cit.*, Morton, *British Finance 1930–40*, XIV, pp. 240–1, *International Currency Experience* (League of Nations 1944), III, p. 61, League of Nations *Monetary Review, 1938–39*, p. 54.

ready kind than the cash ratio, it is true, but dependable enough nevertheless to be used as a basis for monetary policy. Now a ratio is essentially a statistical thing, and it either exists or it does not. The argument is not that all the factors which banks bear in mind are indicated in a balance sheet—which no-one has ever suggested—but that certain groups of items in those balance sheets maintain a rough but recognisable relationship towards one another. That is a statistical hypothesis and it is open to statistical test. If one says, as I understand Mr. Frost to say, that there is no more or less firm convention in this matter which bankers follow consistently, but that *given* a certain volume of liquid assets *and* a certain change in bank rate *and* a certain state of expectations, certain results are likely to follow, then one is really saying that there is no secondary ratio as generally understood but there is a definite liquid-assets—bank-rate—state-of-trade relationship. With the former I am inclined to agree, while I am open to persuasion on the latter.[1] But we are discussing a convention, and in such a case it is a particularly unenviable distinction to be the only man in the regiment in step, because what matters is not which idea is sound or unsound but which idea *is in fact* generally accepted.

The second main criticism advanced is that my analysis is invalidated because no allowance is made for time lags in the adjustment of assets. Specific reference was made to this, of course, on p. 296 of my article. Mr. Frost does not discuss the reasons I gave for ignoring possible time lags and for assuming that, on balance, the effects would more or less cancel out, but I need not repeat them here. I would only remark that the bulk of Mr. Frost's discussion refers to advances, where the period necessary for adjustment may clearly be substantial.[2] But advances *plus* investments is another matter, and it is through investments that banks are likely to make short-run adjustments. While I would certainly not suggest that a wholesale dumping of securities by the banks is a practical proposition, British banks weight their security portfolios (intentionally so, of course) so as to make a running-down of investments, through maturity or sale, a process which can be put into action fairly quickly and without serious loss. If there is one thing which the experience of 1952 has shown it is that the susceptibility of bank balance-sheets to shifts in the gilt-edged market has been much exaggerated.[3] But even this is not all. If the analysis had shown that risk assets did not fall when the liquidity ratio fell, and nothing else, then a good deal of weight might fairly have been attached to this question of timing. But the analysis showed clearly that risk assets tended to *rise*,[1] in the majority of cases because of a rise in investments. Mr. Frost will surely not have us believe that the banker is so much a creature of habit, and so enslaved to sheer momentum, that he cannot stop himself from buying securities except after a delay of several months. And is the banker to be denied all foresight, all ability to anticipate coming changes in his holdings of short-term assets ? Does every decline of his liquidity ratio below the critical level take him completely by surprise and unable to do very much about it for—one infers—as long as fourteen months ? This would hardly be the display of financial acumen which most of us have been led to expect from the City of London.

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[1] Though Mr. Frost will surely have to do better than the single case he puts forward in support of his thesis, which is that in 1919–20 the combination of an increased bank rate and narrow liquidity margins after six months checked a rise in bank deposits and after fourteen months reduced them. No mention is made of the fact that the month when credit expansion ceased, June 1920, was also that in which the post-war boom began to collapse, wholesale prices reaching their maximum and unemployment its minimum. Is this really a coincidence ? Is it seriously argued that the decline in bank deposists beginning in January 1921 originated in the monetary measures of fourteen months previously, rather than the 25 per cent fall in wholesale prices and increase in Trade Union unemployment from 1.2 per cent to 6.4 per cent which occurred in the immediately preceding months, and the general slump conditions which were then developing ?

[2] Even so, one reads rather wistfully of how gently bankers are said to treat overdraft holders and applicants at times of stringency. A good many of us will envy Mr. Frost his banker ! (Contrast with the evidence of Mr. Hyde of the Midland Bank before the MacMillan Committee—*Minutes of Evidence*, Qs. 944–6).

[3] See the discussion of the depreciation of the banks' portfolios in *The Banker*, August 1952, especially pp. 64–5.

May I end by expressing my thanks to the Editor of the BULLETIN for allowing this question to be discussed at what has been—for a journal not primarily concerned with monetary matters—such generous length ? It may seem axiomatic to many academic readers that any reasonably-argued view on an issue of this importance should be permitted full expression. Alas, in fact it is not always so; but that is another story.

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[1] See especially Tables III, IV, VI and VII.